LETTERS TO THE EDITOR

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The Mathematics used in Connection with Physics.

IT may seem ungracious for an author to reply to a review containing so many kind expressions as the one with which Prof. Ayrton has honoured my book on "Electricity and Magnetism" in your issue of November 18; nevertheless, I trust that you will permit me to make a few explanations and even corrections, if the word is permissible.

Since the reviewer states that "it is not quite obvious what is the object of giving" the mathematical introduction, I may state rather more fully than I was able to do in my rather long preface for what class the book was intended. The only class of students with which I come in contact at Clark University is composed of so-called "graduate students," that is men who have taken the bachelor's degree at a college, and are intending to undertake research. It is not generally known that this university was founded for the express purpose of encouraging such men, and consequently, alone among American universities (with the exception of the Catholic University in Washington), has no other students. The same class of students is, however, to be found in large numbers at all the larger universities, so that what I have to say is of general application. These students come to us from all parts of the United States and Canada, and I have had two or three from Europe, so that they have had very various training. They have been at college for four years or more, and have generally taught for awhile themselves. They may have studied the calculus for two years or less, so that although they all know "the meaning of a differential coefficient," and are able to integrate and differentiate with fluency, they are not Cambridge wranglers, and their ideas regarding continuity, convergence of series, definite integrals, and the like, are generally decidedly hazy, while they probably have no acquaintance with the calculus of variations, the theory of functions, and more difficult subjects. It was in order to have some of these matters of most frequent occurrence in a convenient place to refer to that I prefixed the introduction, not with the idea of giving a complete treatment, but to show the student some of the things he should certainly get up, and by means of foot-notes to show him where he could go further. In the numerous kind letters that I have received from teachers of physics in this country, I think all have especially commended the idea of this introduction. At any rate, I had so good an example as Maxwell, who thought it worth while to put a mathematical chapter at the beginning.

With regard to the suggestion that "possibly the students of Clark University, when listening to such lectures as are given in this treatise, have the physical meanings of the various mathematical processes explained to them," which the reviewer intimates would be desirable, I will say that these students have worked generally two years or more in a laboratory, making the usual measurements, and that it is hardly necessary to explain to them what a magnet is, or how a galvanometer is constructed. In order to cover the ground, it was necessary to condense, as the book was already larger than I intended.

Prof. Ayrton asks, "Is it correct to say that 'following the usage of the majority of writers, we shall denote' Laplace's operator (I forbear to write the signs of variation, which the printer has put for round *d*'s) 'by Δ,' seeing that many writers, including Thomson and Tait, use ∇^2 and Maxwell $-\nabla^2$?"

I was rather careful to find this out when writing the passage, and to quote only from the works which I have at hand. I may state that to the two authors named using ∇^2 might have been added the names of Lamb, Minchin, Routh, Basset, and Rayleigh, while the notation Δ is used by Mascart and Joubert, Duhem, Kirchhoff, F. Neumann, C. Neumann, Mathieu, Boltzmann, Helmholtz, Clausius, Drude, Picard, Jordan, Hertz, Klein, and by Poincaré, who calls it *la notation habituelle*. The notation Δ_2 is used by Lamé, Somoff, Boussinesq and Voigt, while Betti writes Δ^2. I did not say "the majority of English writers."

That Gauss's theorem "would be perfectly useless if gravitational, electric, and magnetic forces did not vary as the inverse square," I can hardly agree, seeing that it would be just as true, and would have applications to the flow of heat, hydrokinematics, geometry, and the theory of functions. It is just as well for electricians to remember that the world was not made for them alone.

The reviewer is certainly labouring under a misapprehension when he says "The statement of the general problem of electrostatics, as given at the beginning of § 135, is insufficient since, as pointed out, any number of solutions could be given to it." On the contrary it is explicitly pointed out, at the top of page 265, that there is but one solution, which is completely determined. It is comforting, however, to learn that "the method, however, which is indicated for the solution of the problem is correct, and leads in a neat way to the conception of coefficients of induction"; though the credit can hardly be taken by the present writer, the method being taken from Betti's well-known treatise given among the list of works made use of.

With regard to d'Alembert's principle, I will only say that for the purpose for which it is introduced, namely the deduction of the equations of motion of a system of particles, Hamilton's principle, &c., it seems to me that it makes no difference whether the internal forces appear or not. In any case I have said more about the principle than Kirchhoff, who merely writes down the formula, and Appell, who only says *qui signifie que, pour un déplacement virtuel arbitraire imprimé au point à l'instant, la somme des travaux virtuels de la force d'inertie et des forces reellement appliquées au point est nulle.* Practically the same statement is made by Thomson and Tait, and repeated by Tait in his small book on dynamics.

It seems to me that Green's function is hardly "divorced from its physical application" when it is stated that Green introduced it to solve certain problems in electrostatics. Its physical meaning as the potential of a certain electrification is also given.

The reviewer states that the analogy between electrical and magnetic phenomena is carried too far when the same letter is used for specific inductive capacity on some pages and for magnetic permeability on others. Also that "the beginner might expect to find μ instead of which he finds ϵ; the meaning, however, of this ϵ does not seem to be given." As a matter of fact, it is explicitly stated that μ is used where it refers indifferently to *either* the electric or magnetic quantity, while the meaning of ϵ is given in the next line to the one in which it first occurs, on page 509.

With regard to my "poking fun" at anything or anybody, I beg leave to assure Prof. Ayrton that his statement that "the fun is not intentional on the part of the author" is due to a misapprehension.

Finally, to the suggestion that "a course of 'Lectures on Mathematical Physics' may fitly contain explanations of the physical interpretations of the equations developed without running the risk of appearing to pander to the electrical contractor," it may be replied that much depends upon the point of view. To me the steam-engine or the dynamo are interesting as examples in thermodynamics or induction. I am well aware that this is not the usual view, nor do I suppose it ever will be. I need not conceal the fact that none of my students have ever become engineers. We have an excellent engineering school in our city (which, by the way, is Worcester, and not Webster, as your heading makes it), and we have no reason to try to duplicate the work done there. Some of its graduates have come to us, and have done good work in physics or mathematics, but they have dropped engineering. I say this, not with the slightest wish to disparage engineering or engineers, but to emphasise the fact that there are others to be considered as well.

A. G. WEBSTER.

Clark University, Worcester, Mass., December 15, 1897.

A New Single Picture Pseudoscope.

THE principle of the stereoscope is so well known that it is unnecessary to point out that two dissimilar pictures are required of a special character in order to produce the stereoscopic or solid effect. Consequently it may be imagined that to obtain a stereoscopic effect with a single picture is an impossibility.

It is clear that if the possibility exists a *true* stereoscopic combination would not result, but one which would approximate more or less closely to the truth.

Many devices have been brought out in the hope of giving a single picture a solid appearance, such for instance as a large convex lens. All these devices, however, fail to give the desired result. The illusion, so far as it goes, is simply a distortion of the original picture.

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The nearest approach to the production of the stereoscopic effect from a single picture is by photographing the illustration with its plane inclined at an angle of the optic axis of the lens, first in one direction, and then again in the opposite direction; in which manner two dissimilar pictures are produced, which can be viewed with an ordinary stereoscope, and the result is by no means unsatisfactory. The process, however, is inconvenient.

For a long time past the writer has been experimenting with stereoscopy, and amongst other objects one has been to secure a stereoscopic effect with a single picture. This he has now succeeded in attaining in a comparatively simple manner.

If a stereoscopic slide is examined it will be found that each picture is compressed, so to speak, on the inner side, which is equivalent to saying that the central line of any one picture is shifted from the centre inwards, thus expanding one half of the picture and compressing the other half.

It therefore appeared to the writer that, if by some means a single picture could be made to give two images, each one compressed in a suitable manner and then viewed with the ordinary stereoscopic lenses, the solid effect should be produced, and such is the case.

The method is as follows:—A large deep cylindrical lens is taken and cut in two along the line where the lens is thinnest. Thus two wedges are produced, each having one side curved and the other side flat. If these two pieces are placed together, with the thick portions towards one another, and held at a short distance over any picture, the eyes will perceive an image of the illustration in each lens, and the pictures will be compressed in the manner already referred to.

All that is now necessary is to view these two images with ordinary stereoscopic lenses. One picture will result, as solid in appearance as if the ordinary stereo-slide had been employed.

The apparatus will be found of considerable use for looking at small photographic portraits, landscapes, &c., as well as engravings. The result is very pretty.

Naturally one class of picture must suffer under the operation; but as these are comparatively rare, there is an extended use for the instrument, which is called the Pseudoscope.

The pictures unsuitable are those which represent an object when placed very near the eyes, since the image produced by solid objects in each eye in such cases differ greatly; but this difference in the image is comparatively small after a distance of, say, 15 to 20 feet.

DAVID SALOMONS.

MAGNETIC OBSERVATIONS IN THE HARZ MOUNTAINS.

DR. ESCHENHAGEN has made an interesting series of magnetic observations at forty-two stations in the Harz Mountains.

The uncertainty of a declination observation was about 1′, of the dip with two needles about 1′·2, and the probable error of the horizontal force was about ± 0·00010 C.G.S. units. As it was intended to compare the results with those of a survey of the same district in which the deviations of the plumb-line were determined, the stations were selected with reference to this fact, and also to the geological conformation of the country.

The magnetic disturbances and disturbing forces were determined by a method essentially similar to that employed in the survey of the United Kingdom. The results were not very definite in the northern part of the district examined, but a clearly marked "ridge-line," or locus of attraction on the north-pole of the magnet, was discovered in the south. In one part of this the vertical disturbing force nearly reached 0·00400 C.G.S. units. There are few stations in England and Wales where the disturbance is as large as this, though at Stratford-on-Avon it is exceeded by 25 per cent. At some places purely local attractions of very much larger magnitude were detected. Thus, on the Leistklippe the dip was altered by 11′, and the vertical disturbing force was 0·04224 C.G.S., or one-tenth of the whole vertical force. No such remarkable effects were observed near the principal granitic masses, such as the Brocken.

The fact that the magnetic ridge-line lies to the south of these is explained by the hypothesis that the granitic mass, of which the Brocken is the upper end, extends underground in a direction inclined to the vertical, and running from the surface towards the south. This view is, in the opinion of Dr. Eschenhagen, consistent with the geological facts. As, however, the granite is but slightly magnetic, it is also necessary to suppose that the heavier, more magnetic granite, lies deep, and is only brought near to the surface at a few points such as those at which the greatest local disturbances occur.

The local deviations of the plumb-line were deduced from observations made by the Royal Prussian Geodetic Institute. To the north of the mountains the plumb-line is deflected to the south; from the south an attraction towards the north is detected. The line of no deviation is to the south of the Brocken, and runs nearly parallel to, but about 10–12 km. north of, the magnetic ridge-line.

The remarkable result is therefore attained that (1) geological evidence, (2) the magnetic needle, and (3) the plumb-line all point to the conclusion that the heavier rocks lie some distance to the south of the chief visible masses of granite.

Dr. Eschenhagen is careful to point out that it is unlikely that a similar agreement would be attained in every place where similar experiments might be carried out, as the rocks of greatest density are not necessarily magnetic. None the less, his observations make it probable that the magnet, the pendulum and the plumb-line may add much to our knowledge of the details of the constitution of the crust of the earth; and his survey of the Harz Mountains is one of the most striking attempts which have yet been made to combine the results of magnetic and geodetic surveys.

A. W. R.

THE JOURNAL OF ANATOMY AND PHYSIOLOGY.

NEARLY nine years ago, on the occasion of the coming of age of the above-named Journal, we directed attention (NATURE, vol. xxxvii. p. 441) to its association, as a medium of publication, with the then newly established "Anatomical Society of Great Britain and Ireland." Since that time the connection between the two has been maintained, much to the advantage of the Journal, for not only have the "Proceedings" of the Society materially improved and increased in bulk and importance, but many of the leading papers which have appeared in the body of the Journal have reached it through the mediation of the Society, while in the latter's annual reports of its "Committee of Collective Investigation" there have been contributed results of great service alike to the scientific and surgical anatomist. Owing to the death of the founder of the Journal and main supporter during its earlier years, the beloved and universally respected Sir G. Humphry, a reconstitution of the staff of its "conductors" was recently decided upon, and with the issue for October last a new series was accordingly commenced, Profs. Sir W. Turner and J. G. McKendrick, so long associated with its success, announcing that for the future they would be assisted by Profs. D. J. Cunningham, A. Macalister, and G. D. Thane. There has just reached us the second (January) part which has appeared under these new auspices. During the years which have elapsed since our previous notice was written, three of the five present conductors of the Journal have been presidents of the Anatomical Society, while Prof. Macalister, having been on November 26 elected to that office, now combines the two functions. And during the same period the Anatomical Society undertook the preparation, and the conductors of the Journal the publication, of a couple of analytical indexes to its first thirty volumes, entrusting the task to Mr. A. W. Kappel, the efficient librarian of

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